

battery, or is analogous to a galvanic battery of *many small pairs*; for (1) the body consists of many segments; (2) there are nearly as many nervous ganglions as segments (normally as many); and (3) the animals in the more typical species have extreme rapidity of movement and high instincts. The small number of ganglions in most Spiders is evidently due to a coalescence of several in the one central thoracic ganglion, as in Crabs.

In the highest Mollusks, the Cephalopods (Cuttle-fish, &c.), the Invertebrate *quantity-battery* reaches its greatest power.

Vertebrates also appear to correspond to a quantity-battery (as shown by the simplicity of the nervous system), but to one admitting of vastly greater power.

XX.—On the Value of the "Villi" on the surface of *Amœba* as a Specific Distinction. By H. J. CARTER, F.R.S. &c.

IN Article XIII. of the 'Annals' for August 1863, vol. xii. p. 111, Dr. Wallich calls upon me to account for many things—more than I have time now to answer.

I rejoice, however, to see that he has taken up the study of the freshwater Rhizopoda so zealously, and hope that he may make much progress in it; for, regarding a correct knowledge of these elementary forms of life as, at present, the alphabet, so to write, of organized creation, I shall not be found wanting in gratitude to him for every moment that he may devote, and for the smallest trifle that he may add, to our information respecting the *Amœbæ*; while, if I fail in this, or am guilty of the opposite, viz. of detracting from him, which has not been, nor ever will be, done intentionally, I am certain, on the other hand, that he will obtain that justice and be allowed that priority from those acquainted with the subject, which truth and right in the end always secure in matters patent to public scrutiny. But not being particularly ambitious of such awards myself (as I am for the most part satisfied if I can obtain the publication of anything which I think may be useful, in a truthful form and to the best of my ability), I may perhaps on this account be backward in acknowledging the assistance that I derive from others, where this does not appear to me to be absolutely necessary for the subject on which I may be writing.

I would, however, wish it to be understood that my remarks on *Amœba princeps* ('Annals' for July 1863, p. 30) were chiefly derived from observations made on this species of *Amœba*, in Devonshire, in April last; while those by Dr. Wallich were made in London about the same time on an *Amœba* which he then considered to be such "a well-marked species" that he adds

"I accordingly propose that it should be named *A. villosa*" ('Annals,' May 1863, p. 366). Thus our observations were made independently of each other, and, as I thought, on different species; for I could not conceive then that any one possessing a knowledge of the freshwater *Amœbæ* could confound *A. princeps* with any other form of *Amœba*, or give it a new name without making *particular* mention of this circumstance. Nor had I subsequently any reason to think otherwise, until I had the pleasure of making Dr. Wallich's acquaintance personally towards the end of May, when I learnt from him that he regarded *A. villosa* and *A. princeps* as one and the same; upon which I added this remark to the rough (as it now stands in the printed) copy of my paper (*loc. cit.* p. 44)—not pointedly, at the commencement, but cursorily, in the latter half of the paper.

But what is the value of this "villous surface" on *Amœba*, as a specific character, if Dr. Wallich assumes that "many of the so-called species of *Amœba*, if not all," are mere "varieties" (vol. xi. p. 287, and vol. xii. p. 115) of his *A. villosa*? since they certainly all do not present the "villi." And where is the specific character to determine that all the other *Amœbæ* are mere varieties of *A. villosa* when the "villi" are absent?

Independently of not agreeing with Dr. Wallich in such an assumption, I saw that the presence of the "villi" in *A. princeps* was inconstant, which induced me to lay stronger weight on that which appears to me to be a peculiar form of the nucleus in this species; and until convinced to the contrary, I must adhere to these views. My opinions regarding the specific value of the villi and the form of the nucleus respectively in *A. princeps* will be found in my paper; so I need not repeat them here.

Let anyone conversant with the subject compare my figures of "*Amœba radiosa* (?) Duj." (Annals, vol. xviii. 1856, pl. 5), diagrammatic as these are, with Auerbach's *A. bilimbosa* (Siebold und Kölliker's Zeitschr. vol. vii. pl. 19. figs. 1-10), and see if they are not so much alike that they may be assumed to be the same species. And then let him compare these with Ehrenberg's, Dujardin's, and my own figures of *A. princeps*, and see if the differences between these two *Amœbæ* are not quite sufficient for specific distinction. Yet Dr. Wallich assumes that *A. bilimbosa* and *A. princeps* are mere varieties of one species, of which the typical form is his *A. villosa*, whether they have the villous surface (his chief specific character) or not; while he further adds that "the balance of evidence" appears to be in favour of the "*whole* of the varieties of *Amœba*" being reducible to a single specific type! (Annals, ser. 3. vol. xi. p. 443): the italics are mine.

With these views, then, I saw that it was impossible for Dr. Wallich and myself to get on together in this subject, and

therefore I sent my paper on *Amæba princeps* to the press with the few allusions which it contains to what Dr. Wallich had previously published on *A. villosa*.

As to this paper conveying the impression that "every one of the characters peculiar to the *Amæba* of which Dr. Wallich wrote had been or were "for the first time" brought to notice by myself (p. 115, vol. xii.), that I have not been "just" to Dr. Wallich (*ibid.*), and that I have "assailed directly or indirectly nearly every opinion and statement" that he has made in the communications under reference (p. 151), I certainly do not think that my paper calls for any such expressions, and, without defending myself in long explanatory notes which no one would read, am quite content to receive the verdict which those acquainted with the subject may be disposed to give after reading Dr. Wallich's and my own papers respectively in the 'Annals' for 1863, Nos. 64 to 68 inclusive.

Dr. Wallich (p. 115) has completely distorted the application of the term "nomenclature" as I have used it in the commencement of my paper (p. 30). The passage runs thus:—"It may be remembered by those who have read my 'Notes on the Organization of Infusoria, &c.' [published in 1863], that I have therein proceeded upon a certain nomenclature of *their parts* generally; and I shall pursue the same course here in the description of *A. princeps* specially." Yet, to read Dr. Wallich's objections, it would appear that I had used the word "nomenclature" with reference to *species*. It was not until 1863 that I ever met with *A. princeps* in such numbers as to be able to make so much out of it as I have done of the other freshwater Rhizopods, viz. as regards its "reproductive cells." And as in this paper the *Amæba* are not referred to generally, my remarks must be considered to apply to *A. princeps* specially, where it is not stated otherwise.

Of Dr. Wallich's criticisms on my "Communications" on the Infusoria generally previous to 1863, I must also leave the public to judge; and if I ever carry out my intention of writing more at length on the subject, I shall then, in the general review of what I have written since 1856 inclusive, hope to benefit by what my friend Dr. Wallich has written in 1863.

XXI.—On the Nomenclature of the Foraminifera.

By W. K. PARKER, Esq., and Prof. T. R. JONES, F.G.S.

Part IX.—*The Species enumerated by De Blainville and De France.*

DE BLAINVILLE, in the article "Mollusques" (Dictionnaire des Sciences Naturelles, xxxii. 1824), enumerates several forms